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Certainly it is excellent discipline for an author to feel that he must say all he has to say in the fewest possible words, or his reader is sure to skip them; and in the plainest possible words, or his reader will certainly misunderstand them. Generally, also, a downright fact may be told in a plain way; and we want downright facts at present more than any thing else.—RUSKIN.

Original Communications.

REMARKS ON STETHOSCOPY AND AUSCULTATION.*

BY AUSTIN FLINT, M. D.,

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Laennec, in entitling his great work "A Treatise on Mediate Auscultation," implied the need of an instrument to conduct to the ear intra-thoracic sounds. In view of the discovery of auscultation, together with the extent and completeness of Laennec's observations, it would be ungracious and ungrateful to dwell upon aught which he failed either to observe or appreciate. One can not, however, help being surprised that he almost overlooked immediate auscultation. Not a few of those practitioners who at the present time practice auscultation are content with the application of the ear to the chest, never using a stethoscope. Undoubtedly immediate auscultation answers all practical purposes in many cases; yet without the aid of the stethoscope an exploration

* This article embodies substantially remarks made at a reunion of the New York Library and Journal Association.

of the chest can not be complete, and is often unsatisfactory. The ear can not be applied to all parts of the chest. It is difficult or impossible without the stethoscope to interrogate separately circumscribed sections of the pulmonary organs; and for the localization of cardiac murmurs an instrument which limits the source of conducted sounds within a small area is highly useful, if not indispensable. For these and other reasons stethoscopy must be practiced by one who aims to become an accomplished auscultator. Assuming this, the kind of stethoscope to be preferred becomes an important topic.

The stethoscope devised by Laennec is now merely an object of curiosity. It would seem as if he took for his model the rolled quires of letter-paper which was the instrument improvised when the idea of an acoustic exploration of the chest entered his mind. Laennec's stethoscope was a perforated wooden cylinder about an inch and a half in diameter and nearly two feet in length, the pectoral end having a bell-shaped excavation for pulmonary signs, the excavation being closed with a wooden plug for the sounds of the heart. It was made to separate in the middle for the convenience of carrying it in the pocket. I am so fortunate as to have a stethoscope of this description which once belonged to Laennec himself. It was given to me by a former colleague, Prof. Choppin, of New Orleans, to whom it was presented by one who received it from the hands of Laennec. It is the more valuable as a relic from the fact that it was undoubtedly not only owned but made by Laennec. He was accustomed to make stethoscopes for himself and his friends, and the instrument which I have bears intrinsic evidence of its having been the work of an amateur turner. Stethoscopes made in exact imitation of the instrument devised by Laennec were imported into this country with the study of auscultation, and they were used by those who were the first to appreciate the importance of the discovery. Soon, however, various

improvements were made in the size, form, and style of the instrument, the material used being generally wood; and at length the different kinds of stethoscopes were extremely numerous, almost every one who devoted much attention to the study of auscultation appearing to feel that he was bound to introduce some new modification. There would be no profit in undertaking to describe the great diversity of wooden stethoscopes since Laennec's day. All are destined to become obsolete; and at some remote period in the future, should any one of them by chance turn up, its use will be less obvious than the surgical instruments found among the ruins of Herculaneum.

The flexible stethoscope devised by the late Dr. Pennock was the forerunner of a vastly more important improvement. Pennock's instrument consisted of a metallic bell-shaped pectoral extremity, joined by a flexible tube to a tube of metal, fitted to be introduced into the meatus auditorius. This instrument had certain manifest advantages; but the vastly more important improvement referred to is the binaural stethoscope devised by the late Dr. Cammann. This instrument is now so well known in this country that a description of it would be superfluous. It has several great advantages over other stethoscopes. The greatest of these is the superiority in conduction. Evidently this depends on the simultaneous transmission of sounds into both ears. Observation shows this; the rationale I shall not attempt to explain. The lesser advantages are important—namely, the exclusion of extraneous sounds, the tubes closing both ears, and the facility with which it is used, the eyes of the auscultator being directed to the chest of the patient. Owing to these advantages I have not the least doubt that this instrument will eventually supersede all the stethoscopes hitherto in use.

In remarking on the merits of the binaural stethoscope I feel that I have a right to speak with some authority. The instrument was perfected and given to the profession in 1854.

I began to use it at once. During the winter of 1854–5, when I was connected with the University of Louisville, I was engaged in the analytical study of the signs obtained in health. I devoted a portion of each day to the examination successively of members of the medical class having well-formed and healthy chests, who were complaisant enough thus to co-operate with me in this labor. My explorations were made with Cammann's instrument, as well as with a wooden stethoscope, and with the application of the ear immediately to the chest. Many practitioners who were members of that class will recall a prolonged *séance*, during which the respiratory, vocal, and whispering signs on different parts of the chest were observed, compared, and noted in writing. These studies were preliminary to writing my work on the Physical Exploration of the Chest, with Reference to the Diseases affecting the Respiratory System. In that winter I began also to use the instrument in cases of disease at the Louisville Marine Hospital. I have used it daily since that time both in hospital and private practice. Moreover, I have observed its use in the hands of my private pupils, the number of whom will not fall much short of one thousand. I mention these things in support of my claim to have had abundant opportunity to form a just estimate of the binaural stethoscope.

It is remarkable that Cammann failed to appreciate the full value of his instrument. He advised its use only in certain cases in which the sounds were too weak to be otherwise satisfactorily observed. He advised against its habitual use in auscultation. Directly after his death I was appointed his successor in the Demilt Dispensary. My first act was to make a requisition for a binaural stethoscope. In fact, Cammann seldom used the instrument, the invention of which will render his name always memorable, as I have learned from his intimate friend and associate, Dr. Leaming. He could not therefore have known the usefulness of the instrument,

because its habitual use for a certain period is necessary in order to overcome a difficulty which is at first experienced. This difficulty consists in a humming sound which the instrument itself occasions. After some practice this sound ceases to divide the attention with the intra-thoracic sounds. It is completely disregarded, and no longer causes any difficulty. My experience in private teaching has led me to expect always that at the beginning of a course nearly all the members of the class will prefer to apply the ear to the chest rather than to use Cammann's stethoscope; but before the course is ended the difficulty is to prevent them from using the stethoscope too constantly to the exclusion of immediate auscultation. This difficulty is one of which every one must become sensible after having become accustomed to the use of the stethoscope.

I believe Cammann shared in an opinion which I have heard expressed by others—namely, that the habitual use of his instrument would impair the ability to auscultate without it. The idea is that the acuteness of hearing is diminished by its use. There is no ground for this opinion, but I can readily understand how it is formed. The sounds conducted by the instrument are so much more intense; they are brought out, as it were, in such bold relief, that in contrast as heard with the unaided ear the sounds seem comparatively feeble and indistinct. There is in addition a reason for the relative unsatisfactoriness of immediate auscultation after one has become habituated to the use of the stethoscope—practice of auscultation without it is apt to be neglected. Hence in recommending its use the importance of keeping up the exercise of auscultation with the ear applied to the chest is to be enjoined. To object to the stethoscope on account of the contrast with immediate auscultation seems to me as absurd as would be the disparagement of the microscope because this instrument makes evident the inferiority of vision with the naked eye. And there is no more ground for apprehension that with proper exercise the ear will be incapacitated for hearing

auscultatory sounds by the constant use of the stethoscope than that the ability to distinguish gross appearances of disease will be affected by microscopical studies.

After what has been said it is quite needless for me to add that I am a zealous advocate of the binaural instrument in the practice of stethoscopy. I meet often with practitioners who declare that they have made trial of it, and prefer some one of the old-fashioned stethoscopes, or the ear alone. Whenever I am told this I know that the trial has been too short to warrant a fair judgment. Let any one who is conversant with auscultatory signs continue to use the instrument for a few weeks or months, resolved to avail himself of whatever advantages it may have, and its use will never afterward be abandoned. To the novitiate in auscultation it is of great value, enabling him to acquire a practical knowledge of auscultatory signs with much greater facility than by the application of the ear to the chest.

It is important to mention a reason other than lack of sufficient trial for the failure of many to appreciate the binaural stethoscope. It is that many of the instruments sold are defective in some of the essential points relating to their construction. There are several nice points which all instrument-makers do not understand, and hence many worthless instruments are sold. It may be of use for me to indicate the essentials of a good instrument. The flexible part should be quite flexible, allowing the pectoral end to be moved freely, without any unpleasant pulling of the ear tubes. The movements of this part should also be without noise. The pectoral end should be neither too large nor too small. If too large, in thin patients the edges will not be every where in contact with the integument, which is indispensable. If too small, the area whence sounds are received is not large enough, and the exploration of the entire chest is unduly tedious. The tubes should be unobstructed. It is only necessary to obstruct the tubes leading to one ear to perceive that the

conduction is chiefly made by the column of air within them. The aural ends should have the proper curves adapting them to the meatus auditorius; and lastly, the ivory or rubber knob should not be too large on the one hand, nor on the other hand so small as to enter too far into the meatus; the latter defect renders the use of the instrument uncomfortable and even painful. With no disposition to make an invidious distinction among instrument-makers, I am bound, in behalf of the importance of having good instruments, to say that those made by Tiemann & Co. are almost invariably free from defects, whereas it has happened to me to see more or less of defect in many instruments made elsewhere. I can only account for this by the fact that the first instruments were made by Tiemann & Co. under the personal supervision of Cammann, who impressed the importance of attention to all the nice points involved in their construction.

There have been of late years many efforts to improve Cammann's stethoscope. One improvement which has been suggested is the addition of a piece of parchment stretched over the pectoral end. The conduction seems to be somewhat increased by this addition. Another is surrounding the pectoral end with an India-rubber ring, which extends a little beyond the margin. This originated with Dr. Snelling, of this city, and is an improvement in so far as it facilitates the closeness of the contact of the pectoral end with the integument. Alison has devised a modification, converting the instrument into what he calls the "differential stethoscope." The modification consists in having for each of the tubes introduced into the ears a separate pectoral extremity. Instead of the sounds from one part being conducted into both ears, those of two parts are received separately, but synchronously, the sounds from one part into one and from the other part into the other ear; that is, the sounds from two situations are perceived simultaneously. The increased conduction due to the same sounds being transmitted into both ears at once is

lost in the differential stethoscope. Moreover, it is based on a false principle—namely, that two sounds can be best compared when heard together—that is, at the same instant—whereas a little reflection will satisfy any one that two sounds can be best compared with each other when heard in succession. If one desires to compare two musical notes, they should be heard in alternation, not in combination.

With respect to another proposed improvement suggested by Alison—namely, placing between the chest and the pectoral end of the stethoscope an India-rubber bag filled with water, which he calls a "hydrophone." Five minutes' trial suffices to show that the only effect is to diminish in a certain degree the conduction.

In concluding this article it will perhaps not be deemed amiss to present, with brief comments, some considerations pertaining to the study of auscultation, which are applicable alike to the practice of stethoscopy and the application of the ear to the chest. The considerations which I shall present will have reference to the following inquiry, which, it may be supposed, arises in the minds of all those who aim to acquire practical proficiency in the employment of this method of physical exploration: What are the requirements for success in the practice of auscultation?

1. The first of the several requirements which I shall enumerate is the study of the healthy chest. The auscultatory phenomena in health forms a point of departure for the study of the morbid signs ascertained by auscultation. Some of the more important of the latter are abnormal modifications of normal sounds. How is it possible for the former to be studied until the latter are known? In the category of abnormal modifications of normal sounds are the bronchial, the broncho-vesicular, and the cavernous respiration, increased vocal resonance, bronchophony, amphoric voice, and pectoriloquy; increased bronchial whisper, whispering bronchophony, cavernous whisper, whispering pectoriloquy, and

amphoric whisper. Besides, the adventitious sounds, or râles, can not be fully appreciated without a previous acquaintance with the signs of health. With the several points of disparity between the two sides of the chest, as regards the auscultatory signs of health, one must become practically conversant in order to judge accurately of morbid differences. One of the greatest of the difficulties in effective practical teaching I have found to be an inability to enforce sufficiently the study of the healthy chest as an essential prerequisite for entering on the study of the signs of disease. The more complete the preliminary acquaintance with healthy signs the greater the facility with which a knowledge of the signs of disease is obtained; and indeed without the study of the former the latter can never be fully learned.

2. A requirement for notable success in the practice of auscultation is that knowledge of the signs which is based on an analytical study of them. The study which I would characterize as analytical resolves the signs severally into their differential characters, derived chiefly from points of difference pertaining to intensity, pitch, and quality of sounds. A fuller exposition I have given elsewhere.* If the signs are known only by their resemblance to certain sounds, or simply by a recognition of their peculiarities without analysis, there can not be that fullness and reliability of knowledge which is derived from an acquaintance with their distinctive characters obtained by analytical investigation. For example, suppose the auscultator to have only this general idea of the sign called *bronchial respiration* — it is the sound produced by blowing through a tube. Such an idea must be incomplete and unreliable as compared with the knowledge of the sign based on the following distinctive characters: an inspiratory sound of variable intensity, high in pitch and tubular in quality, followed by an expiratory sound, prolonged, more intense, still higher in pitch, and of the same quality. Take as

* Physical Exploration of the Chest, etc.

another example the *cavernous respiration*, and suppose it to be defined a sound like that caused by air passing into and out of a cavity of greater or less size. With such a definition it is not strange that the bronchial and cavernous respirations are confounded, and that the existence of each as a separate sign has been denied. Let, however, the cavernous respiration be defined as consisting of a low-pitched, non-tubular, and non-vesicular sound, followed by an expiratory sound still lower in pitch and non-tubular in quality; there need never be difficulty in discriminating it from the bronchial respiration.

3. Another requirement is such a degree of familiarity with the distinctive characters of the different signs that they are recognized without mental effort. The signs should be as familiar as household words, so that in the practice of auscultation there is nothing for the mind to do but to reason and draw conclusions. The requisite familiarity is to be acquired by frequently repeating and dwelling sufficiently on the characters which distinguish the different signs. In this respect a practical knowledge of auscultation is acquired in the same way as a foreign language. The auscultator should be conversant with the signs as the linguist is conversant with the words of the language which he has studied with success. If this comparison should seem to represent the study of auscultation as a large undertaking, let it be considered how limited is the vocabulary, all the auscultatory phenomena of disease being embraced in only twenty-four signs. Contrast the labor of becoming as familiar as possible with the distinctive characters and meanings of these twenty-four signs with that of acquiring a mere smattering of any foreign lauguage!

4. Another requirement is that all the varied abnormal physical conditions which are incident to the different intrathoracic diseases should be fully understood. The physical signs represent not diseases, but the altered physical conditions belonging to diseases. The successful auscultator must

know what the numerous abnormal conditions are which give rise to the morbid auscultatory phenomena, else he is certainly not prepared to comprehend the significance of the latter. There are two points of departure for exercises in the study of auscultation, namely: *first*, from the signs, their distinctive character, and their significance; *second*, from morbid physical conditions, and their representative auscultatory phenomena. It is a highly useful exercise to enumerate often all the abnormal conditions incident to different diseases in connection with the signs to which they give rise.

5. The ability to abstract the mind from perceptions and thoughts other than those connected with the observation of auscultatory phenomena is another requirement. Different minds differ as regards the power of directing the attention exclusively for a certain time to the sounds obtained by auscultation. Herein is one reason for the difference among different persons as regards the facility with which a practical knowledge of physical signs is obtained. The ability to concentrate the attention exclusively upon auscultatory phenomena, if deficient, may be acquired by practice.

6. For the successful application of auscultation to diagnosis a tendency to premature conclusions is to be held in restraint. If an opinion be formed of the nature and seat of the disease prior to the physical exploration, the auscultator is apt to strive by means of the latter to corroborate the opinion to which the mind is already committed. This involves a bias which may affect not only the reasoning but even the direct observation of the auscultatory phenomena. He who would practice auscultation with success should be careful to resist a natural inclination to anticipate the result of the physical exploration, deferring his conclusions until all the facts are obtained.

Some of my readers may perhaps be surprised that I do not include in the foregoing list knowledge of the mechanism of the auscultatory signs. This is not an omission through

inadvertency. In the first place, the study of the laws of technicalities of acoustics is no more needed for the practice of auscultation than for either learning to play on any musical instrument or appreciating the performances of others. In the second place, however desirable it may be to understand the manner in which auscultatory signs are produced, ignorance or error in this regard does not affect the practical value of auscultation. The significance of signs is in no measure dependent on our ability to explain correctly the mechanism of their production. To undertake to determine on principles of acoustics the phenomena to which *a priori* certain physical conditions should give rise, or, on the other hand, to infer causative conditions from the characters of the signs, would be as likely to lead to false as to true conclusions. Accuracy and skill in the practice of auscultation are by no means inconsistent with theoretical error. Bean was doubtless a good auscultator notwithstanding his fantastic notions respecting the mechanism of signs. So the consonance theory of Skoda, which pervades his work, does not impair its practical value. Like the tripod from which the priests and sybils of old delivered their oracles, auscultation rests on three supports. One is the knowledge derived from the analytical study of the signs, another is the knowledge of the physical conditions incident to disease, and the third is the knowledge furnished by clinical experience which establishes constancy of connection between signs and physical conditions.

NEW YORK.

RELATIONS OF YELLOW FEVER, MALARIAL FEVER, AND MALARIAL HÆMATURIA.*

BY JOSEPH JONES, M. D.,

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I have endeavored by careful observation of the various symptoms, by analysis of the blood secretions and excretions, and by careful examination of the pathological lesions after death, to unravel the complicated chain of phenomena characteristic of yellow fever and other diseases; and while many facts are unexplained, and much remains to be investigated, we feel assured that the labors which we have pursued unremittingly during the past eighteen years have been at least in the right direction.

The malarial poison induces profound alterations in the constituents of the blood. Under its action, as I have fully demonstrated by the first series of investigations (Trans. Am. Med. Ass., 1859), the colored blood corpuscles are more rapidly and to a greater extent destroyed than in any other disease; the fibrin is diminished and altered in quantity and quality; the albumen is in like manner diminished; the extractive and coloring matters of the blood are frequently increased. The unhealthy hue of the complexion in malarial fever appears to be due to both the destruction of the colored blood corpuscles and the presence of coloring matter in the blood, the deposit of pigmentary matter, and the failure of the liver to separate fully the coloring matter of the bile.

In yellow fever there is no marked or uniform destruction colored blood corpuscles. The fibrin is diminished,

* This brief but very interesting article was addressed by its writer to Dr. R. B. Porter, of Courtland, Ala., in reply to certain inquiries relating to the subject, and was kindly sent to this journal by Dr. Porter.—EDS. AM. PRAC.

and to a much greater extent than in malarial fever. There is no tendency to the formation of fibrinous concretions in the cavities of the heart in yellow fever, while the formation of such laminated fibrinous clots is common in malarial fever, and in some cases is the manifest cause of death.

Malaria, by its effects in inducing sudden congestions, and by its depressing effects upon the heart and upon the general and capillary circulation, and by its potent action on both the sympathetic and cerebro-spinal system of nerves, tends to promote the formation of heart clots, although there is an actual diminution of the fibrin in the blood during malarial fever. In view of the rapid, feeble, intermittent pulse; disturbed, panting respiration; feeble, rapid, fluttering action of the heart; cold extremities, exhaustion of the muscular forces, stupor, wandering of the intellect, inability to control the muscles and acts of excretion; in view of the sudden onset of all the symptoms in malarial fever; in view of the observations which we have carefully recorded heretofore upon the lesions characteristic of malarial fever, we are justified in asserting that the fibrinous elements of the blood may be deposited in the heart and blood-vessels during life in malarial fever, and not only give rise to distinct phenomena, but cause death in cases which otherwise would not have terminated fatally.

It is worthy of observation that in pyæmia and in malarial fever, in both of which diseases there is a more rapid destruction of the colored blood corpuscles than in any other class of diseases, *chills* should characterize both affections, and form the most marked symptom. If such facts do not point out the nature of the cause of malarial fever, they at least sustain the belief that this disease, like pyæmia and yellow fever, is due to the action of a special poison, and not to mere variations of climate and changes of moisture and temperature.

The rapid destruction of the colored blood corpuscles in malarial fever is evident, not only by a comparison of the

constitution of the blood in this disease with that of yellow fever, but also by the presence of a larger amount of coloring matter in the urine. As a general rule, the graver the case of malarial fever the more deeply colored is the urine; while, on the other hand, the reverse is the case with yellow fever. The coloring matter of the urine in yellow fever is due to a great extent to the retention of the biliary matters in the blood and the failure of the action of the liver; while, on the other hand, the deep red and reddish-brown and orange-colored pigments of the urine of malarial fever appear to be derived chiefly from the colored blood corpuscles.

While the presence of the coloring matter in large amount in the urine of malarial fever may be dependent in part upon some imperfection in the excretion of carbon by those organs whose special function it is to eliminate this element from the blood, as the liver and lungs, and may, as has been observed by Golding Bird, be connected with some functional or organic mischief of the liver and spleen, or some other organ connected with the portal circulation, at the same time, from a careful consideration of the accompanying symptoms and subsequent post-mortem revelations, we have been led to the belief that in malarial fever the pigment is derived chiefly from the coloring matter of the blood-cells, and that its amount may be taken as an index or measure of their destruction. This would be true, whether it comes at once from the blood corpuscles by changes taking place in the mass of the circulating fluid, or by the destruction of the blood corpuscles in the liver and spleen. Certain it is that this pigment is not found in the kidneys, and does not accompany diseases of the kidneys; nor is it thrown off under the action of organic medicines and compounds, drastics and purgative salts, which irritate and even cause disease of the intestines and kidneys. Even tincture of cantharides, when given in such large doses as to cause albuminuria and even blood to appear in the urine, does not cause such pigments as purpurine (Bird), uroërythrin

(Heller), or urohæmatin urophæin to appear in the urine. In those cases of yellow fever in which we have the greatest irritation of the kidneys, or rather in which there is the greatest structural alteration of these organs, will be found, as a general rule, the lightest-colored urine. On the other hand, poisonous metallic salts, which derange the constitution of the *colored blood corpuscles*, and interfere with the *blood-making* or *blood-regulating functions of the liver and spleen*, as the compounds of lead, copper, mercury, arsenic, and antimony, cause even in small doses the appearance of this substance in the urine; and when taken in doses sufficiently large to produce poisonous effects the quantity is greatly increased.

In malarial fever the constituent of the blood which suffers to the greatest and most essential degree is the colored blood corpuscle.

In yellow fever the constituent of the blood which suffers to the greatest and most essential degree is the albumen and its modification fibrin.

The peculiar action of the poison in the former upon the colored blood corpuscles induces a distinct train of symptoms, and establishes distinct recognizable lesions, characterized chiefly by the deposit of pigment matter in certain organs; while in the latter the poison causes such changes in the albumen and fibrin as lead to the formation of non-nitrogenous and nitrogenous materials, some of which, as the oil and modified fibrin, are arrested or accumulated in certain organs, as the heart, liver, and kidneys.

During the active stages of both yellow and malarial fever phosphorus and the compounds of phosphorus in the nervous structures, as well as sulphur and the compounds of sulphur in the muscular structures, undergo more rapid changes than in the normal state; and phosphoric acid and the phosphates, and sulphuric acid and the sulphates, appear in increased quantities in the urine when the kidneys perform

their offices. The waste of phosphorus and of its compounds in the nervous structures during the active stages of the disease is greater than the supply of these materials through the food. The nervous disturbances and debility characteristic of these fevers, as well as of others, are in a measure due to those rapid changes in the phosphorescent materials of the nervous structures, and especially of the central ganglionic cells.

In many cases of yellow fever, and in that form of paroxysmal fever called *malarial hæmaturia*, the function of the kidneys is impaired, and neither the urea nor the mineral acids are increased in the urine; while at the same time they accumulate in the blood, and exert deleterious effects upon the nervous system and blood. The increase of the urea and of phosphoric and sulphuric acids during the active stages of these diseases should not therefore be considered as any thing peculiar and as at all distinguishing them from other fevers. It is only the tendency to congestion and alteration of the excretory structures of the kidneys that characterize yellow fever. The peculiar intoxication and nervous symptoms, as well as the black vomit of yellow fever, are intimately associated with suppression of the urinary excretion. In many cases I have found the black vomit of yellow fever to *give a strong alkaline reaction from the presence of ammonia resulting from the urea eliminated by the gastric mucous membrane.* I have also detected by repeated analysis *urea in large amount in the brain, heart, liver, spleen, muscles, and blood in yellow fever.* In this disease suppression of the action of the kidneys is more to be dreaded than black vomit, which it often precedes and induces.

The increase of these constituents of the urine is referable to the same cause—that is, increased chemical change—in both fevers, although it is evident that the nature of these chemical changes, and the special constituents involved, may differ in each disease.

During the slow action of the malarial poison, as well as during the active stage of the paroxysm, important changes take place in the liver and spleen which are wholly different from the changes of these organs in yellow fever. In malarial fever, in both the liver and spleen, the colored blood corpuscles are destroyed in large numbers, and the coloring matter resulting from the disintegration of the colored corpuscles accumulates in them, and in conjunction with other changes in the nutritive processes of these organs produce those characteristic alterations of the normal color. In fatal cases cellulose is found both in the liver and spleen, while grape-sugar is absent from the liver. The *bile* also is altered, both in chemical constitution and physical properties.

In yellow fever there is no destruction of colored blood corpuscles either in the spleen or liver, and no deposit of pigment matter, while *oil* is deposited in large amount in the liver, which, together with the bile, impart to this organ a yellow color far different from the *dark slate or bronze color of the malarial liver.* The spleen is comparatively unaltered in yellow fever. Both cellulose and grape-sugar are found in the liver of yellow fever.

That the chemistry of the body is deranged in a definite manner in malarial fever is evident from the changes of the excretions. *During the chill*, and at the commencement of the hot stage, phosphoric acid disappears almost entirely from the urine. As the hot stage progresses, and the febrile action and the heat commence to decline, there is an augmentation of phosphoric acid. The uric acid is either increased or remains at the normal standard during the chill, disappears almost entirely during the fever, and then increases rapidly and rises to a high figure after the subsidence of the febrile excitement, and often continues for days two, three, or even six times more abundant than in the normal state, as I have shown by a large number of observations published twelve years ago.

The sudden variations in the physical and nervous phenomena of malarial fever are accompanied by equally sudden and marked anatomical lesions and changes in the excretions. No such variations in the phosphoric or uric acids are observed in yellow fever. The poison inducing malarial fever acts in a definite manner, and is governed by definite affinities and relationships, and produces a type of disease distinct from yellow fever. The malaria of the swamps and marshes can only generate paroxysmal fever.

In the vast majority of cases of malarial fever albumen does not appear in the urine. This constituent of the blood may, however, be present in the urine in malarial fever under certain circumstances.

1. Its presence in the urine of malarial fever may be due to preceding disease of the kidneys, of the liver, or heart.

2. To the prolonged action of the malarial poison, and the structural alterations induced by it in the spleen, liver, and kidneys.

3. To the congestion of the kidneys from cold, or from the impaction in the capillaries of pigment matter, or from the irritant action of the malarial poison upon the excretory structures in cases which have suffered with repeated attacks of intermittents.

It is the exception to the rule to find albumen in the urine in malarial fever; it is the exception to the rule to find albumen absent from the urine of yellow fever.

Even in those cases where the prolonged action of the malaria has produced profound structural alterations of the liver, consisting in the extensive deposit of black pigment granules within and around the capillaries of the liver, obliteration of many of the branches of the portal system within the lobules, and in the hardening and contraction of the entire organ, albumen is rarely present in the urine. I have examined the urine carefully without detecting albumen in a number of cases of ascites and extreme dropsical infiltration

of the lower extremities, produced by the hardening and contraction of the liver in chronic malarial poisoning. I have observed cases, however, in which the kidneys were structurally altered by the malarial poison in a manner somewhat similar to the liver, in which albumen was a constituent of the urine.

A certain proportion of such cases may be referred to the causes which ordinarily lead to structural alterations of the kidneys, as the excessive use or abuse of ardent spirits, and the effects of exposure to wet and cold and extreme temperature; but there are cases of albuminuria which can be explained only upon the supposition that they are due to the structural alterations of the kidneys induced by the prolonged action of the malarial poison. And this condition of the urine is not to be referred to the watery condition of the blood induced by the destruction of the colored corpuscles and diminution of the albumen and fibrin; for the state of extreme anæmia frequently induced by the action of the malarial poison is never attended by albuminuria, unless there be some structural alteration of the kidneys.

In that form of malarial fever characterized by complete jaundice, intense vomiting and nausea, and hemorrhage from the kidneys, which has received different names at different times and in different countries, and which is *no "new disease"* even in these southern states, the hemorrhage from the kidneys is *preceded by congestion of these organs*, and is attended with *desquamation of the excretory cells*, and *tubuli uriniferi* of these organs.

Malarial hæmaturia (hemorrhagic malarial fever—new disease—up-country yellow fever), as a general rule, occurs only in those who have suffered from repeated attacks of intermittent fever, or who have been exhausted by a prolonged attack of remittent fever; and while some of the symptoms, as the nausea, incessant vomiting (and in extreme cases *black vomit*), deep jaundice, and impaired capillary circulation, resemble

those of yellow fever, yet there are marked differences, similar to those already indicated, as distinguishing malarial and yellow fever.

The presence of the albumen in the urine of this so-called "malarial hæmaturia" is attended also with the presence of colored blood corpuscles, excretory cells of the kidneys, and the *tubuli uriniferi*, impacted ofttimes with altered blood corpuscles. I have even detected the Malpighian corpuscles containing altered blood corpuscles, and deeply stained by the coloring matter of the blood. As a general rule in yellow fever, the *tubuli uriniferi* are loaded with yellow granular, albuminoid, and fibroid matter.

In those cases of malarial hæmaturia which have come under my observation there was evident congestion of the kidneys, attended with desquamation of the excretory cells and coats of the tubuli uriniferi and active hemorrhage. In some of these cases immense quantities of green biliary fluid was vomited, and the patients died in a state of apparent collapse. As a general rule, suppression of the functions of the kidneys is a fatal sign, and, as in yellow fever, may be attended with convulsions, coma, and delirium. A careful examination of the blood in malarial hæmaturia reveals great diminution of the colored corpuscles and fibrin.

The pathological changes observed after death are characteristic of malarial fever: enlarged slate and bronze liver, with pigment granules; enlarged and softened spleen, filled with disorganized colored corpuscles and pigment granules; gall-bladder distended with thick, ropy bile, presenting when seen *en masse* a greenish-black color, and in thin layers a deep yellow. As much as one thousand grains of bile of high sp. gr. (1036) have been obtained from the gall-bladder, while in yellow fever not more than one hundred and twenty grains of bile are, as a general rule, contained in the relaxed gall-bladder.

I have thus clearly demonstrated that *malarial hæmaturia*

is related to the various forms of true malarial fever (intermittent, remittent, and congestive), and in fact is only one of the phases of this fever which may at any time be assumed after the alterations of the blood, liver, and spleen induced by the prolonged action of malaria. I have also clearly shown that it is distinct from *yellow fever*, although it may have some symptoms in common, as jaundice, black vomit, and albuminuria.

The treatment of malarial hæmaturia should be conducted upon the same general principles which should guide us in the treatment of *pernicious* intermittent, remittent, or malarial fever; with this addition, that attention should be paid to the condition of the kidneys, and they should be relieved by cut cups and counter-irritation. The bowels should be freely opened by a mercurial (calomel is the best preparation) combined with quinine. Ten grains of calomel and ten grains of quinine is a useful combination. Quinine should be freely given. The strength should be supported by nutritious diet (beef-tea and milk punch), given by the rectum if the stomach will not bear it. Alcoholic stimulants should be used without any fear of injury. The action of the skin should be promoted by the hot-air bath and steam bath.

In brief, the *strength must be supported and the paroxysm arrested by quinine; the liver and bowels and portal system must be unloaded; the congestion of the kidneys must be relieved;* and during convalescence the blood must be enriched with pure and nutritious diet and iron, and a gentle action of quinine maintained.

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A PLEA FOR THE TRACTOR.

BY WILLIAM GOODELL, M. D.,

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My purpose is not to offer the vectis as a substitute for the forceps. The far greater power and range of the latter forbids any such rivalry. But I wish to present to the profession a better instrument than the one lying idle in their obstetric bags, to advocate its use in special cases, and to show that its action is mainly that of a tractor. Abroad the vectis is highly esteemed. In Italy, Holland, and Belgium it bids fair to displace the forceps; but at home it has fallen into undeserved disgrace. The cause of this disfavor is owing partly to the faulty construction of the American instrument, and partly to its use as a lever and not as a tractor. As sold in our shops it is too short for applications high up in the pelvis, is shaped to act mainly as a lever, and has too moderate a curve to cling firmly to the head; while the ambitious fenestra in the flat, thin, and metallic handle, made for the purpose of removing the obsolescent globe-pessary, renders the grasp unsteady and painful.

About fifteen years ago I stumbled upon a vectis—Lowder's I believe it is—which has served me so many good turns that I wish to bring it to the notice of the profession.

From this figure it will be seen that this vectis has a smooth, broad blade, a wide fenestra, and a curve much sharper than the one with which most of my readers are familiar. It has the further merit of being provided with an honest wooden handle, long enough for applications above the brim, and round and thick enough

to be firmly grasped. In addition, by a hinge in the shank, it can be folded up into a very portable compass. The shape of this instrument renders it a very efficient hook for traction; and also, by fitting any portion of the fetal head-globe, lessens the risk of its slipping, and consequently of its injuring the mother or the child.* Thus constructed the vectis virtually becomes a prolongation of the four fingers of one hand. It should therefore be used in precisely the same way and for precisely the same purposes as a thumbless hand would be used, could it reach such points on the surface of the child's head as are accessible to the vectis.

Granting then an efficient instrument, let me show the nature of its action, the mode of its application, and the conditions demanding its use.

With Dease, Davis, and Copeman, I believe that the aim of the operator should be to use the vectis as a tractor—viz., as a hook—rather than as a lever. My reasons for this opinion are as follows. In the first place, it is usually in refractory occipito-posterior positions of the vertex that the pure lever-action of this instrument is recommended, and theoretically it would seem to be demanded; but practically, in a large majority of these cases, the vertex will spontaneously rotate anteriorly, provided the head is forcibly flexed, and can be got down low enough to press upon the coccyx or to bulge out the perinæum. Now both these indications are met by pure traction on the occiput by the vectis. Even when the occiput tends to rotate into the hollow of the sacrum the vectis reverses its course not so much by leverage as by its hook-like action; the occiput being, as it were, hooked up toward the symphysis pubis rather than prized up. In the second place, even when pure traction is aimed at, the action of the vectis is a compound one. For, in order to keep the concavity of the blade in close adaptation to the convexity of the head, and

* This instrument can be obtained from Messrs. J. H. Gemrig & Sons, No. 109 South-eighth Street, Philadelphia.

thus to prevent slipping, a certain amount of lateral pressure is unavoidable, which partly converts the action into that of a lever of the "third kind." This inseparable lateral pressure, being proportionate directly to the tractile force, develops the needful amount of leverage—at least so have I found it. Again, if the vectis be used as a lever of the "first kind"—the head being the weight, the physician's hand the power, and the rim of the outlet the fulcrum—the head would be simply prized out or scooped out; and such a method of delivery is plainly at variance with the mechanism of labor, as well as hazardous to the integrity of the mother's and child's tissues.

From these points of view therefore the name *vectis*, or *lever*, becomes a misnomer in so far as it tends to mislead the physician and to misdirect his efforts. Strictly speaking, the action of this instrument, being the resultant of two forces—the one lateral, the other linear—is not essentially that of a *lever*, but that of a *lever* and *hook* combined. Further, its action may be virtually narrowed down to that of a *tractor*. For not only will it be safer to use it in that way, but the cases to which it is best suited are those in which, as will shortly be seen, pure traction alone is needed; the unavoidable lever-action being, in the extractive sense, wholly unnecessary, and in fact thrown away as so much lost power. For these reasons it seems to me best to lessen the prominence of this action. I shall therefore abandon the terms *vectis* and *lever*, and call it a *tractor*—a name which defines the bulk of its action, and which therefore is more technically correct.

The tractor can be slipped on without any change whatever in the usual lateral position of the woman and very generally without her knowledge. Whenever the head has descended so low as to press upon the coccyx the blade may be carried directly to the point on which it is to act; but in applications higher up, as the curve is a sharp one, it will be

necessary for the woman to draw up and abduct her right thigh, while the physician first introduces the blade along the hollow of the sacrum until it reaches the head, then inclines it toward the desired point, and finally adjusts it by sliding it onward, and by at the same time carrying the handle well back toward the perinæum. To whatever portion of the head the indication points there may the tractor be applied; but the face of a living child should, if possible, be avoided. The place of election is the sacral side of the occipital region, the fenestra being slipped over the occipital protuberance; and fortunately, in the majority of cases, here will it best aid the mechanism of labor. Whenever thus hooked on very forcible traction can be safely made.

The extractive power should be exerted rather in the line of the handle than at an angle with it, yet with such an amount of lateral pressure as will keep the blade from slipping. Traction should, in general, be made with both hands, but more especially with the one at the shank, its index-finger or thumb being kept on the head to note the amount of slipping. When the tractor is thus used, since both hands are exerting more or less traction, it is hardly possible to decompose its compound action by any given rule of mechanics. The bulk of its action is plainly that of a hook; and yet a portion of it may be described as that of a lever of the "third kind;" the hand at the shank being the power, the one at the end of the handle the fulcrum, and the child's head the weight. In those cases which demand very little extractive force one hand only may be used, and then the palmar aspect of the wrist corresponds to the fulcrum, for upon it will the end of the handle rest.

Traction should in general be made only during a pain; but in changing a faulty presentation to a more favorable one—that is to say, in correcting a deviation—it should be begun just before a pain, and kept up during its continuance by a long, steady pull. In cases simply of extraction a suc-

cession of short but steady pulls — not jerks — will, unless contra-indicated, prove much more serviceable. The effect of these interrupted efforts is to make each side of the head-globe descend in turn by alternate flexion and extension. The axis of the head-globe thus becomes a lever, while the tractor represents the power applied intermittently to one end of it. This imitates the side-to-side movement of the forceps.

Whenever the exigencies of the case demand the adjustment of the blade on the cheek or the chin, or on any part of the head lying in front of the ears, the force exerted should be carefully graduated lest the child should sustain injury. In view of this danger, and also of the fact that the portion of the head on which the tractor acts tends both to dip into the excavation and to rotate in the direction toward which the concavity of the blade looks, it is of the greatest importance to make an accurate diagnosis of the presentation and position; otherwise the instrument may be unwittingly placed where it might do harm to the child or interfere with the natural mechanism of the labor.

In three classes of cases may the tractor be employed: (*a*) those in which it acts more efficiently than the forceps; (*b*) those in which it precedes the use of the forceps; (*c*) those in which it may be substituted for the forceps. No sharp line of demarkation, however, separates any two of these classes; each one shades off into the other.

In the first class may be enumerated cases of insufficient flexion; such, for instance, as arrested occipito-posterior positions of the vertex, or of rotations of the vertex toward the hollow of the sacrum. Under such circumstances the blades of the forceps can rarely be applied exactly along the occipito-mental diameter of the head. Hence their grip will sometimes be found not stable enough to compel the requisite amount of forced flexion. The resistance of the mother's tissues then becomes the flexing agent, and delivery is accom-

plished by brute force only. The blades also will tend to play upon the head and injure the scalp. On the other hand, if the blades are applied with relation to the curve of the pelvic axis, and not along the sides of the head, a complete rotation into the hollow of the sacrum will in addition be liable to take place. But by the application of the tractor to the sacral side of the occipital region these difficulties vanish. Flexion is compelled, traction is made, and anterior rotation attained; the last both by the hooking up of the occiput toward the pubes, and by the oblique and upward pressure of the blade, which acts in the sense of an artificial perinæum.

In cases of insufficient extension, such as a mento-posterior position of the face, the tractor has proved in my hands a most trustworthy instrument. Passed over the sacral side of the face, it is far more efficient than the forceps in promoting an anterior rotation. The mechanism here is precisely like that of an occipito-posterior position, with the exception that extension instead of flexion is forced. Slipped over the base of the occiput the tractor renders very great aid in facilitating the final flexion of the head after the chin is born. Again, when the anterior lip of the os uteri is swollen, or is wedged in between the head and the pubes, and the fingers fail to slip it up over the occiput, the tractor, acting like a shoe-horn, will at once overcome this difficulty.

In those transverse cranial positions at the brim due to a contraction in the conjugate diameter, whenever the blades of the forceps can not be applied to the sides or to the fronto-mastoid diameter of the head, the following method, recommended by Prof. Fabbri, of Bologna, will be found of signal advantage—of far greater, in fact, than the application of the blades of the forceps over the face and occiput. The tractor passed over the *pubic* side of the mastoid region is at first used as a lever of the "first kind;" the left hand on the shank representing the fulcrum, and the right hand becoming the power by raising the handle toward the pubes. In other

words, each hand acts in opposite directions, the left one mainly to protect the pubes from pressure. As soon as the handle is raised high enough for the blades to secure a good purchase, and also to compress the offending transverse diameter of the head, then traction is thus made: the right hand is kept at rest to become the fulcrum of a lever of the "third kind," while the left acts as the power. This compound action of traction and leverage meets here several important indications. It compresses the head in its lateral diameter, flexes it, and forces its pubic side to revolve around the promontory of the sacrum as the center of motion, to glide over the smooth surface of the pubic symphysis, and to roll over into the pelvic cavity.

The tractor is not only a better corrector of malpositions and malpresentations of the head than the forceps, but it should precede the use of the latter in all such deviations from a vertex or a face presentation as interfere with the application of the blades along the long diameter of the head. In the one the head must be extended, in the other flexed, before the forceps can be used with safety. A typical example of departure of the chin from the breast is the presentation of the anterior fontanel, or sinciput. Usually this condition is transitory, but sometimes it persists. There are also other degrees of imperfect flexion ranging between a frank presentation of the sinciput and that of the vertex, all of which impede more or less the progress of labor. For instance, in my experience, very few cases of transverse cranial positions are uncomplicated with imperfect flexion. Of course in these cases the forceps must be applied over the face and occiput, or along diameters coinciding more or less with the vertical one; but if the tractor be hooked over the occiput and direct traction be made, the head will be flexed, and the forceps can then be properly applied. Often this mere flexion will alone suffice to start the head on; for the delay is owing to the correspondence of the cervico-bregmatic diameter, or of one

of its congeners, with the axis of the superior strait, by which the biparietal diameter is the first to engage in the conjugate diameter of the brim, and by which also the occipito-frontal circumference of the head, in lieu of the cervico-bregmatic, descends parallel to the successive planes of the obstetric canal. The wedge-shaped head presents, in fact, its broadest end first. Now by flexing the head we restore the occipito-mental diameter to a coincidence with the axis of the superior strait, and bring down the thin end of the wedge. The small occipital protuberance will now first engage, followed by gradually increasing diameters until the biparietal is reached. As Hodge has so admirably shown, these are cases belonging to the province of the tractor; for although the deviated head may correct itself, or may by the forceps (*if it holds on*) sometimes be pulled through by brute force, it will be at the expense of suffering to the woman and risk to the child.

The tractor may be substituted for the forceps, whenever the physician can not apply the latter, while the woman is lying on her side, and he has not assistants enough to change her position. When the uterine pains flag or stop the mere introduction of the tractor, or the slight start which it gives to the head, will, in my experience, very promptly awaken them. Again, the tractor does not present so formidable an appearance as the forceps, and most women will permit its introduction when they would object to that of the forceps. Whenever the after-coming head is detained, the tractor will save those precious minutes lost in the adjustment and the locking of the forceps-blades. I do not, however, advise in these cases the use of either instrument; but that of supra-pubic pressure, conjoined with traction, made on the neck and ankles of the child, such as I have elsewhere described (American Journal of Obstetrics, 1871, page 484). To beginners it is always difficult and often impossible, in oblique positions of the head, to introduce and lock the blades of the forceps. Occasionally an expert is foiled in this attempt,

especially when he has been called in after the soft parts have become swollen, or when the child has long been dead, and it is therefore jammed down into the pelvis. The tractor then becomes a very valuable substitute for the forceps, and may indeed obviate the necessity for craniotomy.

But the cases in which, *par excellence*, the tractor can be substituted for the forceps are those of timid women or of unreasonable attendants. What physician has not met with labors in which the head coys on the brink of the brim without engaging; those in which delay is owing to a perverse coccyx, or to insufficient pressure upon the perinæum; in one word, cases in which a very little aid would save hours of suffering? His duty is clear, and he would gladly do it; but the woman is so nervous, or her friends are so prejudiced, that either the mere suggestion of the forceps is out of question, or else instrumental aid is obstinately put off until the patient, friends, and physician are all worn out. Under such circumstances the tractor is worth its weight in gold; for it can be whipped out of the pocket, slipped under the sheet, warmed in the hands, and applied very generally without the knowledge of the patient, and always without that of her friends. Nor when discovered by the patient, as has occasionally happened, have I ever found her disposed to resent the furtive attempt to relieve her. If the head be low down, the chance of discovery will be very slight indeed. If it be high up, the physician should first tell the woman that he is about to help her, and request her to draw up and abduct her right thigh. He then should distract her attention by accustoming her vagina to the presence and movements of three or four fingers, and finally slide in the blade along their palmar aspect. Sometimes ether may first be given; but it should be withdrawn as soon as the tractor is in position, in order to get all the aid possible from the pains.

The history of the tractor shows how covertly it can be used. It was undoubtedly invented long before the forceps,

and yet for many years after Chapman had publicly described the latter the former was not known to the profession. Although confided to several persons "for a consideration," the secret of its invention was kept for over ninety years, and was not divulged until the year 1753, when it was purchased for the immense sum of five thousand louis by two public-spirited Dutch physicians. Bland has, I think, hit upon the true explanation of this long-kept secret. In a very entertaining essay (London Medical Communications, 1790, page 418) he says that the forceps could not be applied "without giving such a view of them as would enable a curious bystander to form some general idea of them;" whereas "the lever might be used not only without the knowledge of the assistants, but even without the woman herself being conscious that any instrument had been employed."

Such then are some of the merits of the tractor. More could be given, but these will, I trust, serve my purpose of awakening an interest in a long-neglected instrument. In this plea I have tried to act the part not of a special pleader, but of an honest advocate. I wish therefore to be understood as striving not to degrade the forceps from the high rank which it deservedly holds, but to vindicate the credit of its more humble helpmate.

PRESTON RETREAT, PHILADELPHIA.

Reviews.

The Transactions of the American Medical Association. Vol. XXIII., 1872.

These Transactions maintain their portly bulk, which proves at least that the Association commands the labor of its members. The volume just issued strikes us as in many respects far superior to the earlier volumes which emanated from our medical congress. The reports of its committees are much shorter, a good deal more space is profitably devoted to the climatology and diseases of many of the states, and altogether its aspect is decidedly more practical than that of most of its predecessors. The appendix, containing the nomenclature of diseases proposed by the committee, would alone render it a valuable acquisition to the library of every studious physician; but the reports on clinical thermometry, on the white-blood corpuscle, on the present condition of vaso-motor physiology, on the production of vaccine virus, on phosphorus as an organismal element, and several others, possess a scientific or practical value which will insure their being read, and will cause them to be preserved for future reference. The meeting at Philadelphia was one of the largest and most harmonious that has been held, and the volume issued as the result of its labors is in all respects worthy of the Association.

The Committee on Medical Education reiterate the complaints and renew the recommendations of nearly all the committees that have gone before them. They are not satisfied with what the Association has accomplished in the way of reform, and insist that it take immediate and decided action,

so as to make itself felt as the head of the profession in the United States. "It must act quickly," they add, "or it will be powerless;" and a part of the action they propose is "an appeal addressed by the Association to the different state authorities, advising that no more charters shall be granted to medical colleges which do not agree to adopt the plan of teaching which this Association shall hereafter recommend."

We have the highest respect for this committee. The members are among the best in every sense belonging to our National Association. They are men of learning, of high moral tone, and of ripe experience, and we enter our dissent most reluctantly from their well-considered views; but in all candor we must say that this proposition strikes our minds as perfectly idle. An appeal to the state legislatures would be as vain, in our opinion, as calling "spirits from the vasty deep." The legislators would laugh at the appeal. Nay, more; if the Association should adopt the suggestions in regard to a high standard of preliminary education, and all the medical schools of the country should acquiesce in the decision, the legislatures of most of the states would readily grant charters for new schools to any squad of ambitious physicians who might ask for them. Nothing, according to our observation, is easier than to procure a charter for a medical college. It is a display of liberality in which our lawmakers especially delight to indulge.

But suppose the plan of the reformers carried out, and all the matriculates of our schools required to present the degree of bachelor of arts, would the number of medical students be diminished? Not at all. The number of matriculates would fall off immensely, and the number of well-educated physicians would be sadly reduced; but there would be just as many practitioners in the country as now. *Docti indoctique*, they would continue to swarm just so long as there was a demand for their professional services. But the people have been quite satisfied with their medical men as a body, and have

had reason to be so; for it may be safely asserted that, take them as a class all over the Union, they are quite up to their surroundings, and no body of men is likely to be much above or in advance of these. The average doctor of America is equal to the average lawyer, the average merchant, the average divine; and the great men in American medicine are the peers of the great jurists, the great divines, and the merchant-princes of the nation. Nor will any attempt at summary legislation make them any more. No one class can be pushed far in advance of any other. Scholars can not be made in a day. They are the product of an older civilization than ours. They come with wealth, and with the time and leisure which wealth purchases.

All the education that a man can acquire assuredly adds to his strength. In one sense it is indeed true, as has been said, that as many languages as a man knows so many times is he a man. All the scholarship of which he may be able to possess himself will widen the measure of his usefulness and add both to the grace and completeness of his life—will, if he be a physician, augment his charms at the bedside and his finish in the lecture-room; and yet we have the temerity to hold that an education which embraces the classics and the higher mathematics is not essential to a thorough acquaintance with even the highest and most intricate branches of the science of medicine. We go further, and affirm that such scholarship is not essential to the most eminent success in medical authorship—we might add, in authorship generally—and could easily adduce a long list of instances proving the truth of our assertion.

Conceding, however, that the lament of the old men of the profession at the decline of learning is well founded, we discredit altogether the power of the American Medical Association to remedy the evil by any legislation within its reach. Our learned brethren who write so freely about the right of the Association to "demand," "require," "exact," "enforce"

a certain standard of qualifications in students of medicine would seem to fancy that it has the power of the civil government to enact laws, and that it is backed by the gleaming bayonets of the government for carrying them into effect. The moral influence of the Association is great. Whatever it may recommend will be most respectfully considered by the profession, provided it appears that the suggestions have been well matured; but the Association will lose sight of its prerogatives, and of the true sphere of its usefulness, when it assumes to give laws to the profession.

We have contended, and we think with reason, that in the profession of medicine, as in the other professions, the standard of education is, on the whole, about as good as the general state of society permits it to be—about as good as the masses ask it to be. We have held—and we think our position is a tenable one—that the average character of the people determines the degree of education demanded in its physicians; and we believe, with Herbert Spencer, that if it were possible to change the system of education without the average character being first changed, evil rather than good would follow. We further agree with that most logical and philosophical writer that "in all its effects, learning the meaning of things is better than learning the meaning of words;" and that, whether for intellectual or scientific training, "the study of surrounding phenomena is immensely superior to the study of grammars and lexicons."

The preliminary education of medical students may be safely trusted to time and the new conditions which arise in its progress. It has been said that "until men have grown up to the level of a higher belief they can not receive it; nominally they may hold it, but not virtually." It is so with education. Any positive legislation, such as has been so often asked, in the direction of controlling it will effect nothing. The "constitutional conservatism of mankind," as it has been called, will prevent any sudden change in educational systems,

even though all those who would alter and mend and reform them should first agree (which they have never yet done) on what those changes should be. The needs of mankind will oblige education to keep pace with its progress and advancement in other directions. It has done so hitherto in the profession of medicine. It is fair to assume that it will continue to do so.

We beg to commend to those excellent gentlemen who are continually deploring the decadence of scholarship in our profession, and who are so impatient for some active interference from some source, the familiar maxim, *festina lente.* In fact, we would go so far as to hint the favorite motto of Sir Robert Walpole, *quieta non movere;* and if they must disturb the quiet and move things, we beg that they will not attempt it through federal legislation. The best rule in statesmanship is said to be "not to govern too much;" and the wisest rulers have been those who have observed this axiom. The American Government was greatly exercised for some years by the Mormon question. Many enthusiastic and well-meaning people urged one solution and another of the problem. The improvements in engineering and mechanics which made a railway to the Pacific possible, and the enterprise which constructed it, settled the fate of Brigham Young and his followers. To cite another example:

The question of the return of the banks to specie payments in this country has excited almost interminable discussion since the close of the war. Thousands of persons who were engaged in finance have demanded that the paper promises-to-pay be withdrawn from circulation and coin be substituted for it. The wiser people can already see that it was most fortunate for the country that the attempt to do so was not made. The question, as every great question of the world does, is solving itself. Gold to-day commands only a small premium, and in a very few years more the increased productions and industries of the country will so approximate the

values of coin and paper that one can be substituted for the other without creating disturbance in the currency.

Whatever may be the conditions and the abuses in our medical schools and in our system of medical education, we believe that they are gradually disappearing, and that time and public opinion will reform them, as "gradually all things right themselves." Signs of progress in every region of our country are evident to our minds. Let us cite a single example. About twenty-five years ago a few thousand adventurous men, allured by the tales of gold which was said to glitter under the setting sun, braved the perils of a trackless desert, around which hovered hostile savage tribes, and, reaching the shores of the peaceful sea, planted the banners of the civilization which they had abandoned and laid the groundwork of the great commonwealth of California. In a little while these pioneers were followed by their wives and sweethearts. In time the prattle of children was heard in their dwellings. Then came the field-school, then followed the public school, and now there is laid there the foundation of a University which, when completed, will rival in extent and equal in endowment the most ancient and renowned of the universities of the Old World.

One by one the other states will in time follow the example of California; and in the mean time our common-school system, which every one must see is driving private seminaries from the field, is slowly working out the result so greatly to be desired. Education is carried by it to every hamlet and to every door, and a mental culture more generally diffused than has ever been known in any country will be the fruit of this system; and then the preliminary education of students of medicine will become what we should all rejoice to see it; and the people, cultivated themselves, will have a keener eye, it may be, to the literary attainments of their medical advisers. We speak doubtingly as to this last result. We are not sure that many people will not continue to argue with the noble-

man, spoken of by Sir Henry Holland, who, when remonstrated with by his family for employing an illiterate physician, replied that "he thought a man who was so profoundly ignorant of every thing else must certainly know a great deal about medicine." But if the people are educated, the doctors coming from them must be educated too. Meanwhile what the people demand are physicians skilled in the cure of disease; and if they can find where the schools are that have museums, hospitals, and teachers for developing their sons into expert practitioners, there they will send them to be instructed. Science, learning, aptness in teaching, and professional experience and skill are sure to prevail in the long run against pretension, ignorance, and sham; and so the mushroom concerns so much deprecated by the friends of education will finally vanish, and the schools that rest on a sure foundation will remain.

Normal Ovariotomy. By ROBERT BATTEY, M. D., Rome, Ga. From the Atlanta Medical and Surgical Journal, Sept., 1872.

Dr. Battey's patient was twenty-three years of age, and only twice in seven years had any monthly flow. Vicarious menstruation sometimes occurred, and thrice retro-uterine hematocele followed by abscess. Amenorrhœa persisted in spite of well-directed constitutional and local treatment. The patient's health was breaking down, and death seemed not distant, when Dr. Battey conceived the bold, and for this purpose original, treatment of removing the ovaries, a treatment which was successfully executed.

It seems a sad commentary upon the inefficiency of our art that a disorder imperiling life could only be cured by the removal of healthy organs, and these organs, too, essential to womanhood.

Dr. Battey states that so far as his means of information enable him to judge "this operation is *unique* in the annals

of surgery, the nearest approach to it being in the celebrated case of Percival Pott;" and he narrates the case, familiar to most of the profession, where this eminent surgeon removed the ovaries because of their forming a hernia in either groin. Yet, if history be true, those ancient kings of Lydia, Andramys and Gyges, preceded Pott and Battey in making female eunuchs. This practice too—if Dr. Roberts, as quoted by Dr. Tilt, is to be believed—has been known in modern times in India. The following passage from Dr. Peaslee's recent work is also of some interest in this connection: "We are told by Wierus in his book, *De prestigiis*, quoted by Boerhaave, De Graaf, and others, that an Hungarian sow-gelder, nearly two hundred years ago, being disgusted by the lewdness of his daughter, exercised his professional skill upon her also." But we hope a man's medical orthodoxy will not be tested by requiring him to believe this legend.

We have no doubt that the operation of Dr. Battey, bold and successful, will attract the general attention of the profession; but we question the propriety of designating it *normal* ovariotomy.

T. P.

Clinic of the Month.

DIET AND REST IN ABDOMINAL DISEASES OF CHILDREN.—Dr. Henry Blanchard, of Dorchester, Mass., recently read a short but very interesting paper (Boston Medical and Surgical Journal) on abdominal diseases as observed by him in the summer of 1870, from which we make the following extracts:

"With children suffering from disturbed bowels my general custom has been to nourish as far as practicable, no matter how young the patient, and to give nutriment in as condensed a form as possible.

"I usually allow children in their summer sickness, when suffering from thirst, as they are apt to do, a supply of cold water; of course using discretion as to quantity, state of stomach, etc., making it sometimes too the medium of such medicines that are appropriate, but which in a less diluted form would be repelled on account of taste. But cold water *alone*, and in full supply, has been often attended with the happiest consequences. I will refer to a case I had three or four years ago. This little child, between two and three years of age, had been sick for many days with severe bowel complaint, accompanied with almost constant vomiting. No food, no liquid, so far as tried, would remain in the stomach. A teaspoonful of water or any other liquid was vomited as soon as swallowed, and yet the thirst was tormenting. Emaciation was fast going on; the dangers of death seemed imminent. I determined to try the experiment of letting the little patient have her fill of pure cold water. I thought it could not make

matters much worse. A large pitcherful was called for, and a good-sized goblet filled and presented to the child, which was all swallowed with simply stopping once to rest and breathe. Another goblet was filled after a little delay, which was also drunk with great relish, the little patient only stopping once to rest and breathe. Then another was offered, which was seized with less eagerness; and after swallowing about half of it she looked up with what seemed a satisfied and grateful expression, put out her little hand and pushed back the glass as it was again offered to her, lay down in its mother's lap, and soon fell into a quiet sleep, followed by gentle warmth and perspiration; had no more vomiting, and was immediately afterward convalescent, requiring little further medical care.

"I leave it for others to explain why this little child, whose stomach was in such a state of irritation that it could not retain a single tea-spoonful of any liquid, not even water, could yet drink, with only an occasional stop to breathe, within the space of five to ten minutes, nearly a pint and a half, and yet retain it all. I do not mean that this treatment can always be so successfully and safely pursued in cases of bowel complaint, when vomiting is so persistent and thirst so urgent; but I believe that by carefully watching children in this disease, and by careful discrimination, we may often hit upon the very article they most need, and by giving it do a great deal toward a successful issue. In any case of a similar kind as related I should repeat the experiment, trusting that though the gentle stream but increased the flame, the flowing torrent might extinguish it.

"In some cases of bowel complaint I have found cold water, or a weak solution of soda in water, the only substance that the stomach would bear. When this condition obtains in the early stage, while the strength of the child is good, and as yet there is little or no emaciation, I have allowed it thus to be treated, absolutely nothing else being administered for

more than one week, when the stomach has become quiet and capable of retaining and digesting the food.

"As illustrating the advantage of patiently watching children with a view to learn their wants, a case may be stated in which I accidently discovered a longing for sugar. A little had been dropped into the mouth after something unpalatable had been given. I saw that it was sucked down with avidity, and an appealing look cast up for more. The difficulty here had been, as so often happens, to keep any thing down. I at once had a quantity of loaf sugar cut into small bits, and the child fed with them. As fast as one was dissolved it was replaced by another. From one to two ounces was thus taken in a day with great zest, which with a tea-spoonful of water occasionally was all that was used for several days; when tiring of the sweet it was found that other articles could be tolerated which before had been entirely rejected.

"At the risk of being somewhat tedious, I wish to give other instances illustrating that craving for something which the mute sufferer can not indicate, and which it too often, I believe, fails to obtain. A child, after a lingering bowel complaint with teething, seemed far gone, and the stomach rejected every thing offered. The case seemed hopeless. I had known raw beef recommended in some forms of bowel complaint. I determined to try it, and it was soon procured, scraped into pulp, salted, and given at first in small quantity. It was soon apparent that the child had found what it was craving. It was sucked down with all the eagerness it was capable of manifesting, and for nearly two weeks the child took almost nothing else. Convalescence was then so far established that a successful return was made to the ordinary articles of food.

"A very remarkable and, as it proved, a very interesting case was that of a child, about two years old, who had contracted diarrhea of a bilious type, and had, as the mother said, the chills. It had been long sick. On visiting it I think

I found the most emaciated specimen of humanity that I ever saw that was afterward restored to health. With diarrhea and vomiting of every kind of nourishment offered, it seemed a case hopeless of relief. Rather as an experiment than with an expectation of success, I directed a trial of brandy, milk, water, and sugar, made as strong of brandy as could be swallowed. Much to my gratification the child manifested a decided relish for it, and did not reject it. It was diligently persevered with, and the longer it was used the more it was craved and the better enjoyed. Soon a gain of vigor was perceptible in the child. Suffice it to say this was substantially the whole nourishment used until it had consumed three gallons of the spirit. At the end of from six to eight weeks so vigorous had it become that it was deemed expedient to wean it from its stimulant, and to substitute some other and safer form of nourishment. So strong had the love of brandy become that this was found no slight task. The little fellow, as he began toddling about on his feet, was for a long time earnestly calling for 'bandy, bandy.' After months, however, he was apparently weaned from it.

"As further illustrating the whimsical cravings of children, I will mention a case just now recovering from a bowel complaint of about five weeks' standing. The child, something over a year old, one month ago, and before being weaned, commenced with a watery discharge from the bowels and vomiting. The mother, suspecting for a certain reason that her milk was disagreeing with her child, had it taken from the breast. It had been accustomed to being fed from the table at times. Weaning, however, seemed not to have the effect of curing it. The disturbance of the bowels continued somewhat aggravated; the stomach remained irritable; it persisted in rejecting all the various liquids we are accustomed to prepare for similar cases, and became much emaciated. Having found nothing that seemed to meet the demands of the stomach, I directed that the child should be carried to the

table at the family meals, and to be watched for some indication of its wants. In this way it got access to some mashed potato, which it seemed to relish, and which also was not thrown from the stomach. The next article for which it seemed to show a desire was boiled cabbage, small quantities of which were also eaten with relish. A day or two afterward I happened to call on the family at one of their meal hours, and I saw my patient eating with great relish—picking it up with its fingers—little bits of meat and potato hash. The child is rapidly convalescing on this kind of diet. I should add that the child had been suffering exceedingly with teething, and that its gums were seasonably scarified.

"The few cases of dysentery which have come under my notice the past season have been of a mild form. I do not propose to say any thing of medical treatment in this disease. I will state my experience in the use of one article with which I indulged my patients, and which I also used the preceding summer. It was an indulgence that I found gave them no less surprise than pleasure. I allude to the use of watermelon, which, as is well known, we have had in great perfection and great abundance the past season.

"It is never difficult to persuade a patient to take what the appetite craves, though he hitherto supposed that the article was entirely prohibited. I have rarely seen patients enjoy any thing more thoroughly than they have the melon, and I have seen only the best results from its use. I need not say to be successful it should be in a perfect state, neither too ripe nor insufficiently so. In typhoid fever I have been in the habit of allowing it for many years, always finding it much relished, a good febrifuge and diuretic, and a valuable means of allaying thirst.

"I desire to bear my testimony to the importance and value of one other principle in the management of diseases under consideration. It is to be regarded of paramount importance; namely, quiet of body—*rest*. Highly important in

most diseases, it is indispensable in some. I do not fail to enjoin it most strictly in all cases affecting the abdomen. Even in cases not grave in character a quiet, recumbent posture greatly facilitates recovery. I dislike to have a dysenteric patient rise from the bed during the course of the disease. Children in their summer-complaints are equally benefited by a recumbent posture and entire quiet. I know it is not practicable to carry out this management with children always, but much may be done by perseverance and patience in this direction. Tossing babies about in the manner of some nurses and mothers, with the idea of easing their suffering and quieting their cries, must be looked on as an evil and useless habit. Cradles and other rocking machines should be condemned as worse than useless. The quieter children are kept the less suffering and the less demand for medicine. I feel it should be our constant aim and study to reduce medicine to the minimum, being sure that in the perfection of practice we may dispense with the use of drugs to a far greater extent than most physicians are ready to believe at the present day.

"I would not have any one present imagine for a moment that I undervalue the ordinary treatment in the summer-complaints of children. It is to exceptional cases—cases which are not amenable to the usual remedies—that I have desired to call attention. I wish to urge the importance of finding out by any way we can in these protracted and often intractable cases sometimes met with what the patient craves; and if we can discover this, let it have a trial. Instinct may prove wiser than reason. In practicable cases it is better to let the child select for itself. The general disgust that the child, after after weeks of sickness, acquires for almost every article of nourishment afforded in the nursery, makes this desirable. It may reject the very thing it craves when it sees you trying to force its use. Let it indicate its wants as far as possible, as in a case I above alluded to, and we shall be far more likely to obtain the desired result. Let various articles of food be

placed before it; then let the parent observe the pointing of the finger and the suggestive sound it will attempt to utter, and we can hardly fail to meet its wants. We have already seen that it is of less importance what the article is as that it meets the craving of the child. I do not expect this plan will succeed in every instance, but that it will do so accasionally my own experience, and no doubt that of many others, has abundantly proved."

On Death from Chloroform.—Mr. Erichsen, in a recent clinical lecture on this subject, said:

"Thanks to the somewhat frequent occurrence of the heading, 'Death from Chloroform,' in the papers, the subject is beginning to excite considerable amount of interest and apprehension among the public. In the profession there has been, I think, rather a tendency to avoid the subject—to look upon the occurrence of an occasional death from chloroform as a sort of necessary price paid for the advantages of anæsthesia. Whether this be the case—whether the fatal result really depends on an inexorable fate or on some more preventable cause—is, I think, worth inquiring into. Considering the extensive and general employment of chloroform for even the most trivial of surgical operations, a death directly due to its influence is happily of rare occurrence. During the twenty-five years that I have been attached to this hospital I have only witnessed one such death. It is a sight which must always produce a deep and painful impression on those present; the more so, since the administration of chloroform is by no means a necessary part of the operation. The relief of pain is in many cases nothing more than a luxury.

"All surgeons will agree with me that in extra hospital practice especially the administration of chloroform is that part of the operation which often gives most anxiety to the operator. In a hospital chloroform is generally administered by some one who is in the daily habit of performing that duty.

The process is watched by competent observers, and there is every appliance at hand in case of need. In private practice it is often given by the practitioner in charge, whose only experience is derived from a limited use of the drug in midwifery cases, and both the patience and peace of mind of the surgeon are upset; for indeed a considerable amount of practice and experience are required to enable a man to chloroform well. Some acquire the necessary skill more easily than others, but no amount of care can make up for the want of a certain amount of practice.

"We must confine our attention to deaths which are *directly and immediately* due to chloroform. These may occur in three ways—by the lungs, by the head, or by the heart; from asphyxia, from coma, or from syncope.

"1. *Asphyxia* may be caused, first, by tight clothing, or by any thing which hinders the respiratory movements, as a large abdominal tumor; second, by actual choking. If chloroform be given too soon after a meal, vomiting of semi-digested material is very likely to occur, and the larynx may be obstructed by it. A gag or false teeth may slip into the throat and cause choking. The tongue also, like the other voluntary muscles of the body, becomes paralyzed when the patient is fully anæsthetized, and by falling back into the throat may itself cause choking. Deaths from these causes, the first more especially, have occurred most often in dentists' practice. Simple asphyxia is not often actually fatal, though narrow escapes are common. The signs are so well marked that they can scarcely be overlooked, and the patient can generally be recovered if assistance be at hand.

"2. Death from *coma* due to chloroform is rare; still the only case with which I have been personally connected was due to this cause. The patient was suffering from chronic Bright's disease, and had slight symptoms of uræmia. The administration of chloroform had not proceeded far when convulsions occurred, followed by coma and death in about

an hour. In this case, at all events, the mode of death was evidently predisposed to by the poisoned state of the blood.

"3. We come at last to that part of the subject to which I wish more particularly to direct your attention—the death from *cardiac syncope*, as it is called. This is to me a very puzzling mode of death, as difficult to account for as it is to guard against. I presume that by the term 'cardiac syncope' is meant atony and failure of the heart's action. This is an easy explanation, but not altogether a satisfactory one. Among these numerous experiments which have been made with chloroform I do not know of any which prove that it has any direct syncopal action on the heart, or even any indirect toxic action on that organ through its nerves. Still there is no doubt of the fact that people do die without much disturbance of respiration, without becoming distinctly livid in the face. The pulse fails; that is the first thing noticed, and they are dead.

"Now my own idea is these are really cases of asphyxia; that the heart is secondarily, not primarily, affected; and my explanation would be as follows: after death we find in these cases a weak, fatty heart; the valves indeed healthy, but the walls thin, and the muscular tissue pale and degenerated. Now chloroform has always, especially at first, a slight asphyxial tendency. The patient calls out that he is choking, tries to pull away the inhaler, and breathes deeply, then struggles and holds his breath for a few seconds. In a healthy man this soon passes off—the inconvenience is merely temporary; but with a fatty, enfeebled heart it is different. The patient holds his breath for a few seconds, the right side of the heart is soon filled, there is weak propulsive power, the organ can not recover itself, and the result is fatal.

"Some years ago I made numerous experiments on death by asphyxia in animals; and I found that if once the ventricular action were stopped—if a contraction were missed—it was most difficult to start again; generally all was over. And

this is what happens in these cases: the ventricle can not be emptied quickly enough, the rhythm is lost, and almost instantaneous death ensues. The patient then does not die from the direct action of the chloroform on the heart, but from the effect of a slight asphyxial condition, which is inseparable from the administration of this agent on a disorganized heart. After death you may not find the right side of the heart greatly engorged; first, because great engorgement is not necessary to cause a fatal result; and secondly, the blood, even many hours after death from chloroform, is found unusually fluid, so that the dependent parts of the body are congested and the heart is left comparatively empty. Then also artificial respiration is always set up in these cases, and this tends to diminish the cardiac plethora.

"Finally, a few cautions. Never give chloroform without first thoroughly loosening the dress; if possible, not within four hours of a meal; the head should be moderately raised; the pulse, respiration, and color of the face must be carefully watched. As regards the occurrence of the rigid spasm already adverted to, I do not think that sufficient attention has been directed to this condition. A patient with an enfeebled heart is then in a most dangerous, even critical, state; the chest-walls are fixed, the lungs are filled with chloroform-vapor, which becomes diffused but can not escape; the pulmonary circulation is obstructed, and the pressure on the right ventricle rapidly increased. Let the patient partially recover, then give it again slowly; watch for any blueness around the mouth, etc. If the pulse fail, draw forward the tongue with forceps, and set up artificial respiration at once; but prevention is the main point. When there is well-marked asphyxia, artificial respiration is most useful and generally successful; but in these cases of so-called 'cardiac syncope' it is generally useless. The heart has been barely able to meet the ordinary requirements of the system; it can not recover lost ground; it gives in, as it were, at once."

Fall of Temperature in extensive Gun-shot Wounds. The Dublin Journal of Medical Science gives an abstract of a memoir by M. Redard on the themometry of gun-shot injuries, which contains the following: 1. In great injuries by fire-arms fall of temperature is a constantly observed fact. 2. Several elements come into play in producing this fall. Among the principal we will mention nervous shock, the excitement of the combat, with consecutive stupor, hemorrhage, and lastly, alcoholism. 3. Every wounded man brought into an ambulance with a grave wound, which seems to necessitate an operation, and who shows a temperature below 35° 5′ (95.9 F.), will die, and ought not consequently to be operated on. 4. Every wounded man in whom a salutary reaction is not produced within four hours, and by whom the reaction is not a direct sequence of the fall of temperature, must be considered as very gravely injured. 5. Burns give rise to an exceptionally great fall of temperature. 6. The same is the case in wounds of the abdomen. The fall is the more marked the nearer the wound approaches the stomach. The diagnosis of penetrating wounds may become less difficult on account of the characteristic thermometric phenomena to which they give rise. 8. The state of intoxication in which the wounded are sometimes found favors singularly the observed fall of temperature. 9. Wounds by shells, all other things being equal, produce a fall of temperature more accentuated than those by balls.

Ricord on Syphilis.—The veteran French surgeon's recent visit to England is said to have been prompted by his great personal regard for his old friend and distinguished pupil, Mr. Acton, who was to read a paper before the British Medical Association on the *Treatment of Syphilis.* When Mr. Acton had concluded the reading of his paper, Sir Wm. Fergusson introduced M. Ricord to the Association, who, after acknowledging the hearty welcome which he had re-

ceived, spoke (London Lancet) concerning syphilis as follows: There was one great question in regard to syphilis, and it was this, Could it be cured radically? In former times all venereal affections, no matter what, were considered as belonging to syphilis; and certainly there was then an immense number of radical cures by mercury or any other means. In this way swellings of the glands, soft chancres, even warts, and other things not belonging to syphilis, were easily enough cured—radically cured; and there were no after-consequences, no secondary symptoms. This explanation would account for the immensely large number of cases of (reputed) syphilis which used to be radically cured. But since syphilis had been correctly diagnosed, the inquiry to which he had devoted a large part of his life was to see what belonged to syphilis, and what resembled it without belonging to it. There had been great differences in the results of treatment, so much so that a doubt had arisen whether real syphilis could be cured. That doubt as to the curability of syphilis was not recent; it was a doubt which old authors had expressed; and one particularly, with a curious name, which they would probably remember—"Mercurialis"—thought that now and then an armistice might probably be made with syphilis, but that there was no real cure. In fact, they frequently saw that a long time—months, years—after the symptoms had been treated new symptoms appeared; and so the doubt whether syphilis could be radically cured, or whether the cure was only temporary, with a prospect of the symptoms returning, might still remain; he (Ricord), however, had established the law of the unicity of the diathesis of syphilis. The law of syphilis was the same as the law of small-pox, cow-pox, or measles. A man could have but one attack so long as the disease remained in the constitution; that was to say, according to his opinion, a new attack could not take place while the system was still under the influence of the old diathesis. Well, it was exactly so with syphilis. As long as a patient was laboring under

the diathesis of syphilis another *infection* of syphilis could not occur; it was impossible. For instance, after indurated chancre, and the appearance of secondary symptoms, it was not possible for the patient to contract a new indurated chancre, with swelling of the glands, manifestation of skin disease, and so on. After one attack the patient could not have another infection as long as the influence of the first remained in his body. A second contagion could not take possession of the system at the same time. If perchance something of the kind took place, the symptoms would not follow the regular evolution. So when a patient had constitutional syphilis, if a new chancre appeared to be hardened, they would not find the glands swell, or the early manifestation of skin disease appear; and so of other symptoms. Superficial ulceration might take place, just as a spurious form of vaccination might arise on one who was still under the vaccine influence; but it was not a true case—it was not attended with the sequelæ. But if the constitutional disease were cured, if the syphilitic disposition were completely eradicated, then the patient would be able to contract a fresh indurated chancre, with all the subsequent symptoms. If this were the case—and he had observed it with great care, his experience dating back forty years—it proved that syphilis could be cured; and if syphilis could be eradicated, to ascertain whether a patient was cured or not when all the symptoms had disappeared, there would be nothing else to do (though he knew that could not be done) but to try inoculation from an indurated chancre. If vaccination did not take, they were sure the vaccine disposition continued; if it did not continue, vaccination could take effect. In regard to syphilis, the proof had not been carried to this extent; but he had been able to observe that as long as the syphilitic influence continued a patient could not contract an indurated chancre anew, and that consequently, if cured, a new infection might take place. This was a great point gained in science, and it proved

what he had said, that syphilis could be radically cured. Now, as to the treatment of the disease, he would first speak of the treatment of the first stage; that was to say, the primary sore. As soon as he had ascertained that there was a hardened chancre, with a swelling of the glands—not inflammatory, because the glands in this case never suppurated—he immediately instituted the mercurial treatment. There was one point on which there was some difference of opinion: many believed that it was impossible to prevent the accession of the secondary symptoms, the first manifestation of constitutional disease; many thought that no matter what treatment was employed the sequelæ would appear. Well, he had ascertained that if the treatment were soon begun and well carried through, the bursting out of the first secondary symptoms, the roseola, the swelling of the glands of the neck, etc., might be prevented. If this were not frequently the case, it was because the treatment was resorted to too late, when the disease had had time to take root, and secondary symptoms were about to show themselves. In such cases it was not astonishing that secondary symptoms should appear, and the treatment ought not to be blamed. If the treatment were steadily continued, they soon disappeared; but if the treatment were begun early, the observation of forty years gave him the assurance that secondary symptoms would not appear. When secondary symptoms had appeared the best treatment was mercury. If they wished for a perfect cure, this treatment must be continued. In general it was not persisted in long enough. It was dropped as soon as the symptoms disappeared, or a short time after, and then it was not astonishing to see them reappear. But if the treatment were continued five or six months, having regard at the same time to sustaining the constitution in general, relapses would be found to be infrequent. He observed very few cases of relapse, and there would not be many when the treatment was well kept up—when the patient had patience enough and

the physician sufficient courage. After six months of that treatment and no symptoms reappearing, then the treatment with iodine must be begun, and continued for five or six months more. When a patient went to him he said, "You will have a year's treatment; do you consent to that?" "Yes." "Very well; we will go on. If not, good-bye."

There were cases in which syphilis occurred in a healthy person; the only disease was syphilis. Then treatment was very easy; the case was a simple one; they had but one enemy to fight; all went on regularly. But unhappily in many instances syphilis was not alone; there was something else—scrofula, skin disease, scurvy, low constitution, poorness of the blood. They must understand that such complications as these altered the case. The treatment did not act so powerfully as it would do in the first case, as many of these complications were aggravated by the treatment. For instance, syphilis and scurvy might co-exist; and the characteristic of the latter was poorness of the blood, while that of the former was a plastic condition of the blood. Here therefore was a counteracting influence to the treatment for syphilis. Now one thing must be known. In many instances syphilis became the secondary consideration, and they must begin with the constitution of the patient, as debility was the disease that required first treatment. They must attack the strongest enemy first. Syphilis was sometimes quiet, and stopped and waited till they came to it. So when they had improved the constitution they might commence with the treatment, and they must begin by treating the constitutional complication. The best treatment was the proto-ioduret of mercury. The stomach bore this well in general. Sometimes it gave rise to a little diarrhea, which was an easy thing to moderate; but when the stomach was not tolerant of the remedy, a capital treatment was rubbing-in. If this were not an unpleasant and disagreeable operation, certainly it would be in general about the best; he himself should prefer it. In rubbing-in the action

of the remedy was powerful and quick, and the stomach was not at all troubled with it. If it were not so disagreeable, and were a thing that could be done without being noticed, he should give it the preference. However, there were cases in which the skin was otherwise affected, in which there was a skin disease, and then friction could not be used. In a case of complication of syphilis and herpes rubbing-in could not be resorted to. In general, patients bore the iodide of potassium well, and in large doses. For his own part he frequently employed forty, sixty, eighty, even a hundred grains a day, and more. They must bear in mind that if they gave too small doses to some patients they would have no result. It was a remedy that passed through the body with great rapidity. He had had great experience with it, and he had found that in half an hour it had passed away in the urine. Iodide of potassium was a sort of broom of the blood. So they saw that the methodical treatment was this: mercury, iodide of potassium. But only one for the first stage, and only the other for the later stage of syphilis? No; the rule was absolute that as long as there were secondary symptoms well marked mercury must be given; when there was a mixture of secondary and tertiary symptoms, mercury and iodide; for tertiary symptoms, iodide. To treat some patients with iodide would not advance them in any way. Why? Because there was frequently in the constitution, in the blood, something of the second stage, something that required the mercurial treatment. This might not show itself; but when iodide of potassium ceased to do good, the disease remaining stationary, let them go back to mercury again, and they would have a splendid result where they had thought there was no further possibility of curing the patient.

But there was another thing. When syphilis had lasted for a long time, and had had a great effect on the constitution, it in some way disappeared, and left the patient with a complication existing that was not existing before. Sometimes

a long course of treatment brought on a new disease—wasting of the constitution, poorness of blood. They must then stop all the specific treatment, and, applying themselves to the principal symptom, restore the constitution by preparations of iron, bark, tonics, and proper food; so bringing the patient back to the possibility of undergoing anew a regular methodical treatment, either by mercury or iodide, or a combination of these two remedies. In former times, when a person was thought to be syphilitic, physicians seemed unable to entertain any other idea than that of syphilis, and acted exclusively against a specific disease, neglected every thing else, and in that way they experienced all the bad effects and accidental symptoms which a bad administration of the symptoms would produce.

Mr. Acton had spoken of the use of bromide of potassium. Was bromide of potassium an anti-syphilitic remedy? He did not believe that it was. He might be mistaken; but he had experimented with it in syphilitic symptoms, and without any apparent result. But it was a splendid remedy in complications of syphilis. In some cases of symptoms referable to the nervous centers bromide of potassium was an adjunct, and came to the help of mercury or the treatment by iodine. In some cases of brain disease with syphilis, and of disease of the spine or epilepsy, bromide of potassium did wonders. So that they would see it was a remedy to be applied in nervous complications that might occur, but they must not depend on it as an anti-syphilitic remedy. Now there were symptoms following syphilis which were not syphilitic, and these must not be treated with mercury or iodide of potassium. For instance, there might be necrosis. Well, they could not bring a dead bone back to life, no matter what quantity of mercury or iodide of potassium they might give. A physician must know these things, and he (Ricord) ought almost to apologize for bringing them forward. It should be observed that specific remedies did not always act specifically.

Certainly there was no specific effect without a specific cause; but specific causes did not always act specifically. So there were some effects of syphilis, such as disease of the bones, that would afterward act as a common irritant. In syphilis there might be an ulcerated bone in the nose or mouth bringing on suppuration. Mercury or potassium would not remove that; but let the diseased bone be removed, and the patient was frequently cured. They must take note of all these conditions—the nature of syphilis, the manner in which it conducted itself, and its action on the constitution. Let them particularly take note that the general law of syphilis was the same as the general law of small-pox, vaccine, and measles. If they were sure of this from what he had said, and from their own experience, then they might be sure that syphilis could be perfectly, radically cured. They could tell their patients that, and give them courage and hope. If the patient had courage to go through with the treatment, and the physician had courage enough to stick to it, the patient might be radically cured.

A question was asked whether Dr. Ricord was a believer in salivation.

Dr. Ricord replied: No, surely not. Salivation was an accident following the treatment, and it must be avoided as much as possible. There was but one case in which he approved of salivation, and that was in disease of the eye—iritis. When this occurred, and salivation was brought on, the inflammation of the iris subsided.

Dr. Gross asked whether the soft chancre was capable of contaminating the constitution.

Dr. Ricord said his opinion was that a soft chancre, when accurately diagnosed, never gave rise to constitutional disease. This was a law as absolute as possible. But they must be careful, or errors of diagnosis might be made. It was not always easy to establish the difference between soft and hard chancre; but when the diagnosis was certain they might be

sure they would not have any constitutional disease after the soft chancre. On the contrary, even as long as six months after hard chancre secondary symptoms would appear. This was one of the most clearly-established facts in practice. But the hardness of the chancre was not always well marked. It might be very superficial in those varieties that were attended with excoriation. When there was a something like parchment at the base, a chancre was very easily taken to be soft, but was not so; and he had had cases sent to him as instances of soft chancre which had been followed by secondary symptoms, but which were well characterized by the parchment-like base. However, there was a symptom of more value than the parchment base, a symptom that was one of the most important witnesses to constitutional affection, and that was the non-inflammation of the glands; they were cold and dull. In general several of them became enlarged. It was very seldom that only one was found to swell after hardened chancre; and not only were the glands swollen, but the enlargement frequently occurred on both sides, in both groins. The enlargement of the glands was of much value as a characteristic of hardened chancre. The enlarged glands appeared very early, even during the first fortnight of the existence of the sore. With the soft chancre the glands did not always swell. In a great many cases there was no swelling. They would never find a real hard chancre without swelling of the glands; and they would also find many cases of soft chancre with swelling, these cases depending upon surgeons confounding the hard chancre with thickening dependent upon inflammatory infiltration of the tissue immediately around the sore. But if the glands should swell after soft chancre, it was probable that suppuration would come on. With hard chancre there was no inflammation and no suppuration. The older writers directed their efforts to cause an indurated sore to suppurate, in the belief arising from the practical observation that when a bubo suppurated there was no constitutional dis-

ease, and therefore they were under the belief that the poison was thrown out of the body. In their quaint way of putting the fact, "they did not like to shut up the wolf within the fold." But they could not bring on specific suppuration in the case of indurated glands. It was impossible. He had tried all means of doing it, and could not succeed in the cases of specific suppuration. In the instance of soft chancre what had they to do—await the occurrence of suppuration, which might either be attended by simply inflammatory or specific bubo? With the soft chancre the inflammatory bubo appeared sometimes two, three, or four weeks after the occurrence of the chancre, and it had the characteristic pus of the soft chancre. There was such a difference between hard and soft chancre that it was difficult to make a mistake. When a patient consulted him (M. Ricord) suffering from soft chancre he said to him: "Be quiet; you may have a bubo; that will suppurate, but your constitution will be unaffected; you will not be liable to secondary symptoms." With a hard chancre he could predict indurated glands, attended by constitutional symptoms, within six months, provided proper treatment were not followed. He would add that when it was decided that the case was one of hard chancre or soft chancre the treatment was very simple. When there was a doubt as to the nature of the chancre he waited until some characteristic symptom arose. But there were cases in which the existence of a soft chancre did not prevent a patient from contracting a hard chancre. The patient might have the two species at the same time, contracted from different sources. The two species, hard and soft chancres, do not depend upon the difference in the ground, but on a difference in the seed (*contagium*). So that the newcomer who had relations with a woman suffering from the two species could take his choice. If the patient had a true indurated chancre, and well-diagnosed secondary symptoms, he might catch the soft chancre as often as he pleased, and it would be unattended with specific constitutional disturbance.

Notes and Queries.

THE VASO-MOTOR DOCTRINE.—Dr. Samuel Wilks, in the Address on Medicine, at the late meeting of the British Medical Association, says: "Next to the evil of allowing some metaphysical idea to influence our mind is the fault of taking a scientific fact or principle, and making it explain a number of obscure phenomena without our possessing any data to guide us as to the correctness of the application. With one a chemical theory is made to solve all abstruse questions in physiology and pathology; with another, cell-growth can do the same; and with a third a nervous influence can accomplish all things. See, for example, what a remarkable experiment of Bernard has done for us. He divided the sympathetic in the neck of a rabbit, causing the temperature to rise and the part to become more vascular. It seemed clear from this experiment that the vascular supply is dependent on nerve influence, and thus we are introduced into a new domain of vaso-motor and trophic nerves. What has been the consequence? Every ingenious man has sat down in his study, and assisted to flood medical literature with explanations of physiological action, morbid processes, or the therapeutic operation of medicines founded on the theory. Nearly every disorder of the nervous system, be it of an apoplectic or convulsive nature, as well as a large number of other morbid phenomena, can be now accounted for on the theory of an altered vascular supply through the vaso-motor nerves. The operation of many drugs is now made apparent by the influence they exert on nutrition through these nerves, the only difficulty being in the fact that the medicines have different

actions, whereas the vessels are only susceptible of dilatation or contraction. It is for this reason that theoretical men have found strychnia and belladonna the two great agents for rousing and depressing the nerve centers, thus enabling them to hold in their grasp the maladies supposed to be dependent on their rule. This great idea has been much simplified by substituting heat and cold, and thus these nerve-governing powers of the human body being stimulated to increased action by the one, or their over-activity controlled by the other, every single complaint to which the human body is liable can now be cured, and all by means of spine-bags. I take this as one example of our eagerness for more knowledge: there are very few on this earth who are presenting us with new facts, or rather revealing to us some hitherto hidden secret of nature; and when they do bestow upon us the result of their labors, we grasp it, we hug it, we can not make enough of it, until in the end we all become ridiculous. What we want are more facts and more truth. The praise can not be too great which is bestowed on those who are silently working in their laboratories for our advantage. It is more light and knowledge that we want.

Dr. Osborn's Address.—Dr. T. C. Osborn delivered an address to the Medical Association of Alabama on retiring from the chair of president, which we have read with unmingled pleasure. It fully sustains the high literary and professional reputation which the author has earned by more than thirty years of honest devotion to medicine. Such addresses leave their impress upon the bodies to which they are delivered, and impart a loftier tone to professional sentiment and ambition. It is with a feeling of pride that we mark the efforts of so many medical societies in the South, besides that of Alabama, to give higher dignity and usefulness to our profession.

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* Any of these works may be procured on application to Messrs. John P Morton and Company, Louisville, Ky.

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